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ABSTRACT

Conducting observational research in a cross-cultural context invokes a unique set of difficulties. This paper describes sources of bias and threats to validity that are specific to cross-cultural research. It describes problems of cross-cultural research designs for descriptive studies, comparative research, and experimental research and outlines techniques for dealing with problems. (Contains 12 references.) (LMI)

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A GENERAL OVERVIEW OF BIAS AND VALIDITY ISSUES IN CROSS-CULTURAL RESEARCH

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Abstract

Sources of bias and threats to validity specific to cross-cultural research are examined. The paper is intended to provide a general orientation to these issues when evaluating cross-cultural research design. Problem areas associated with descriptive studies as well as with comparative and experimental research are considered. Outlines of techniques and methods useful in dealing with the identified areas of difficulty are also suggested. Twelve sources are cited primarily from cross-cultural research methodology texts and anthologies in the behavioral sciences.

Conducting research in a cross-cultural context has its own particular set of difficulties associated with it. Higher costs, language barriers, inadequate postal and communications services, restrictive social norms, political limitations and unforseen cultural and social factors can all contribute to the general problematic of cross-cultural research. In spite of these barriers, scientists from the various social and behavioral sciences do engage in cross-cultural studies for a number of ethical and personal reasons (Brislin, Lonner & Thorndike, 1973).

"Any theoretical orientation, or any construct, axiom, postulate, or corollary in the social and behavioral sciences can be investigated cross-culturally" (Brislin et al., 1973, p. 143). Reasons for conducting research in cross-cultural settings include (a) the description and categorization of behaviors as conditioned by living in a given country or culture (Brislin et al., 1973), (b) comparative studies aimed at increasing the predictive range of a hypothesis already demonstrated in the researcher's own culture (Lonner & Berry, 1986) and (c) the measuring of beliefs, attitudes and values for the implementation or evaluation of social programs (Segall, 1986). Although researchers engaged in each of these kinds of studies will face similar difficulties in conducting studies in a cross-cultural context, there are also

methodological problems which are specific to each of these types of research in this setting.

Becoming aware of problem areas particular to cross-cultural research is a first step in avoiding the pitfalls inherent in this kind of activity. The application of proven cross-cultural research methodologies developed by veteran researchers in this arena, is another way to insure that studies incorporate certain safeguards in their design. The purpose of this paper, then, is to identify areas of bias and threats to validity which are specific to cross-cultural research and to identify techniques or methods which will be useful, particularly to the novice, in conducting satisfactory cross-cultural studies.

Observational Methodologies

Descriptive research has a wide range of cross-cultural uses. Perhaps the best know of these applications is the ethnographic study, the traditional domain of the cultural anthropologist. These studies are characterized by a description of group behavior and the institutions of a particular culture or society. The majority of these studies are inductive in nature. The anthropologist, theoretically speaking, assumes nothing about the culture and he/she is there to observe and record the group's behavior in its natural environment. Although some ethnographic

studies can also be conducted through literature reviews and analysis (Lonner & Berry, 1986), the majority of this kind of investigation involves field work.

Field work, however, is not the exclusive domain of anthropologists. Political, social and behavioral scientists all employ field methods in the collection of data, particularly when conducting cross-cultural research. Munroe & Munroe, (1986) describe three kinds of information which are sought through field-based methodologies.

1. The collection of basic data such as that obtained through conducting a complete census of a community, gathering information regarding (a) household organization, (b) education, (c) languages spoken, (d) religious affiliation, (e) degree of acculturation, (f) occupational histories, (g) socioeconomic status, (h) fertility and (g) kin relationships.
2. Ethnographic content for a given cultural group's institutions such as their (a) techno-economic systems, (b) social organization, and (c) conceptual framework.
3. Naturalistic data pertinent to specific research variables. This data, such as reputation ratings on member's personal and social attributes which are made by a reliable

source, can be correlated to social and physiological phenomena in the community.

While the first of these three types of information gathering require survey techniques, the second and third use primarily observational procedures. Observational methods are used both in descriptive research and in experimental studies. Bochner (1986), lists seven specific problem areas inherent in using observation as a means of gathering data. These problem areas are not exclusive to cross-cultural studies, but this setting does increase the difficulty of controlling these factors.

1. Reactance. When human beings are observed, they react. Behavior is therefore affected by the act of being observed and thus the "natural" behavior is affected. The only practical solution is to "spy" on the subjects which raises a number of ethical questions.

2. Contextual effects. Human behavior is a function of where they are located at the time of observation. To observe a subject in all of their "natural" settings in nearly impossible and the researcher must then select those of greatest importance.

3. Distortion of the "natural" stream of behavior. To overcome contextual effects, some social scientists have

employed time-and-event sampling techniques to measure predetermined behavioral categories in a number of different settings. This approach has been criticized on the grounds that it distorts the natural flow of events. The opposite approach, that of recording everything, is criticized for leading to mounds of unmanageable data.

4. The internal/external distinction. The interaction between observable behavior, underlying mental processes and underlying physiological processes, and the ways of assessing and measuring each have implications on the kinds of observations to be made and recorded.

5. The base line problem. While in the physical sciences, many measurements can be reduced to some quasi-absolute value, in the social sciences, this is harder to do. Behavior must be measured against a comparison group whose relevant characteristics are known. Cross-cultural studies attempt to extend the baseline beyond the confines of one society and the control conditions are difficult to achieve in these designs.

6. The relationship between variables over time. Many research questions in the social sciences are concerned with how two variables are related over time. Thus, to establish

relationship such as age and performance, the investigator may test several groups which differ according to age. To draw conclusions about how two or more variables co-vary, researchers must ensure that they include sufficient measurement points to their design and that these points are suitably spaced.

7. Individual differences. Variance in human subjects is very great. Subjects will respond differently to theoretically identical stimuli depending on their age, sex, socioeconomic background, etc. This variance can be compounded exponentially in cross-cultural studies. The problem is to uncover how these differences systematically interact with the stimulus of interest. A second challenge is treating individual differences as extraneous variables and concentrating on the "pure" effect of the stimulus, something which is only achievable with large N's, randomizing subjects to conditions, statistical juggling, etc. But sampling all of the cultures of the world in order to test the universal application of a hypothesis, is not feasible in most studies.

As this list suggests, using observational methods in cross-cultural studies is extremely challenging under the best of

circumstances. In discussing the historical weaknesses of these kinds of studies, Bochner (1986) suggests that observational methods have tended to be used by social scientist who have employed less than rigorous methods in collecting data. Two general problem plague these kinds of efforts.

1. The researcher is usually highly visible and this has an effect on the subjects being observed and their behavior. This effect (reactance) is heightened in cross-cultural contexts where observer and observed are culturally quite distinct.

2. Because this kind of research is usually only incidentally interested in confirming hypotheses, the selection of subjects and their observations tend to be less standardized and therefore, less concerned with quantifiability and statistical testing. The results and conclusions tend to be based on the subjective interpretations of the investigator, especially when there are cross-cultural differences between the observer and the observed.

Bochner (1986) suggests that these problems can be overcome by effective research strategy and an appropriate methodology. The research strategy should include an exploratory study that (a)

identifies the target behavior, (b) quantifies the target behavior, (c) uses sampling of subjects and the time periods of observation and (d) forms a clear hypothesis to be tested. Through the confirmatory studies, the investigator wants to study and predict actual behavior in a natural or "real" setting. These purposes are best served by directly measuring these acts. Thus, systematic observation is called into service.

To overcome the problem of reactance, Bochner (1986) discusses a methodology which he and others call the "unobtrusive" method (Webb et al. cited in Bochner 1986). This methodology aims at (a) selecting a suitable setting for the study, (b) selecting a suitable method for recruiting the subjects, (c) determining how the dependent variables will be defined and quantified, (d) devising a procedure for processing the subjects through the investigation at an efficient rate, and (e) recording responses without either the subjects or the general public becoming aware that there is an experiment being conducted. These techniques (along with an understanding of the problems they are attempting to overcome) are particularly important to those conducting comparative studies between cultures.

Comparative Studies

The application of observational methods to behavioral science disciplines has resulted in a blending of the anthropologist's techniques with hypothesis testing. Comparative studies in the cross-cultural context are usually designed to extend the predictive range of a hypothesis already demonstrated in one culture. The researcher uses many individual observations to build a case for generalizing a social or psychological law or theory across many cultures (Lonner & Berry, 1988). This usually involves drawing conclusions from an "emic," intra-cultural study, and testing them for applicability in an "etic" or cross-cultural study.

One problem associated with this type of comparative study is that of perception (referred to as the "emic-etic" or "insider-outsider" dilemma) (Smith & Tayeb, 1988; Brislin et al., 1973). When conducting an emic - intra-cultural - study, the researcher's insider orientation may permit him/her to interpret behavior in the same way the subject or group interprets it. When conducting etic - cross-cultural - studies, the researcher does not have the insider's advantage. Behaviors may appear totally different to those performing them than to the researcher observing them. Smith & Tayeb (1988) attribute some of these differences to problems of translation and non-equivalence of measures. "But some differences

stem from the fact that members of a given culture share complex systems for decoding meaning in one another's behavior that may be inaccessible to outsiders" (p. 154). Thus, an outsider may not be able to accurately observe or measure the behavior of the culture being studied, an obstacle which continues to challenge the cross-cultural investigator.

Berry (as cited in Brislin et al., 1973) suggests the following procedure to combat the emic-etic dilemma.

1. Study only those aspects of behavior that are functionally equivalent between cultures.
2. Descriptive categories derived from past research, perhaps in only one culture, can then be applied to the behavior in the other culture under investigation. This is the imposed etic.
3. The imposed descriptive categories must be adapted so that they explain the cultural practices of the culture from their point of view. This is the emic description.
4. Shared categories can then be used to build up new categories valid for both cultures (derived etic) and can be expanded to other cultures until a universal is established.
5. New instruments and research techniques can be devised and validated using the derived etic as a conceptual base.

These instruments must, of course, be equivalent in meaning to the members of the cultures under study. (pp. 24-25)

Empirical based correlational methods for eliciting the meaning of concepts in the cultures under study have been developed (Nowak, 1962; Przeworski & Teune, 1966; Berrien, 1970 as cited in Brislin et al., 1973). After a careful preliminary study, a set of questions can be formulated around some construct which has "identical" meaning in the cultures under consideration. Once these common questions are found, additional questions may be derived which are specific to each of the cultures. These questions are designed to produce information that is culturally specific. The answers to the culture specific questions are then correlated to the derived set of "identical" questions.

Poortinga & Malpass (1986) describe a process of classifying cross-cultural "universes" which can be used to define scales for the sake of comparing cultures. "For example, one could ask anthropologists and missionaries to draw up lists of the 100 most important plants and animals for any local region in the world and declare scales based on theses lists as adequate comparison scales" (p.24). Comparison scales can be defined when all elements in the respective universe are known and either the entire universe or a representative sample is used for its measurement.

These techniques, while theoretically feasible, are tremendously difficult to carry out in practice. These difficulties tend to greatly limit the topics which can be studied and may call into question the conclusions derived from comparative studies.

Experimental Approaches

Cross-cultural research is subject to the same kinds of threats to internal validity as intra-cultural research. But because it is usually being conducted in field settings under "different" and less than ideal conditions, avoiding bias and reducing threats to validity may take much more effort than the same study in the researcher's own culture. In addition to these "known" factors, there may be unknown or unanticipated problems that arise due to the foreign context of the study.

Campbell (as cited in Brislin et al., 1973) lists fifteen factors that may jeopardize the validity of any explanation of obtained data. Using the case example of a study on parent-child relationships, Brislin et al. (1973) list these factors with cross-cultural examples.

1. History. Another societal variables, such a socioeconomic status, may affect questionnaire responses rather than the variable of parent-child relations in which the investigator

is interested.

2. Testing. Members of some cultures have had more experience taking paper and pencil tests than others, and this experience might affect scores on tests taken subsequently.

3.Maturation. Subjects in one culture may be more likely to become fatigued in a one-hour session than subjects in another.

4. Instrumentation. Observers of different cultures may have response bias habits which might affect the data. Translation inadequacies constitute instrumentation errors.

5. Statistical Regression. If cultures are selected on the basis of their extremity on a dimension, this might affect responses to a dependent variable that has less than a perfect correlation with the independent variable.

6. Instability. This factor refers to fluctuations of observed responses due to unreliable tests or chance factors in samples. Statistical tests are properly applied to investigate this (but only this) factor. In cross-cultural work, as in all social science research, statistical tests are applied to see if obtained results can be interpreted as being *not* due to such chance fluctuations.

7. Selection. The subjects an experimenter selects may be chosen because they are more cooperative and talkative. The cultures selected may be chosen because of convenience rather than scientific interest.

8. Experimental mortality. This factor refers to "losing" subjects or respondents. Subjects in different cultures may be differentially difficult to schedule for a second interview.

9. Selection-maturation or selection-testing interaction. This more complex factor is a possible alternative explanation to the observation that members of one culture increase in amount learned over time, while those of another culture do not. It could be that members of one of the selected cultures were more likely to learn because of their past experience with similar situations or because they were more likely to mature naturally during the course of the experiment.

10. Interaction effects of testing. A pretest given to member of some cultures may sensitize them so that they feel they know how to respond to the experimental variable. This sensitization, because of the pretest, may be less or greater in other cultures.

11. Interaction of selection and experimental treatment. The selection biases, discussed in number seven above, may yield a sample of very cooperative subjects in one culture but suspicious subjects in another. They may then respond differentially to the experimental treatment, thus making generalizations and data interpretation difficult.

12. Reactive effects. This factor refers to the "I'm being studied-how should I act?" phenomenon (as discussed in the section on observational methodologies).

13. Multiple-treatment interference. If subjects are in an experiment or testing session with more than one part, the first part might effect the second, and thus generalization to the responses of people in another culture to only the second part becomes impossible.

14. Irrelevant responsiveness of measures. A questionnaire given in several cultures may contain items meaningful in one culture but strange or meaningless to members of another. . .

15. Irrelevant replicability of treatments. In an experimental treatment with many variables (e.g., a ten-hour reading session which includes pleasant teacher, television, and culture-related teaching devices), a replication in

another culture may not have the same effect because the key variable that caused the original effect may be inadvertently omitted or is less salient to members of the second culture. (pp. 17-19)

This list of possible threats to validity sets the stage for the major difficulty in interpreting results of cross cultural research; the elimination of alternative explanations or "rival hypothesis" in a given set of outcomes (Poortinga & Malpas, 1986; Brislin et al., 1973). At every stage of the study, the researcher should be aware of and list potential reasons that could explain data in addition to the preferred hypothesis being tested. These explanations can be substantive (another cultural difference) or methodological.

There is no technique for eliminating these rival hypothesis wholesale. They must be approached one-by-one and eliminated through the introduction of supplementary (systematic) variation within one or both of the comparison populations. This approach calls for designing research in such a way that the problem is addressed through several dimensions and the findings in each of these correlated with each other to substantiate (or call into question) the preferred hypothesis.

The designer of a carefully planned study should be able to rule out all threats so as to support the preferred interpretation of cultural differences. If a researcher does this, he/she should be able to answer questions that are raised about the interpretation of the results. But in order to do this adequately, several sets of data will probably need to be gathered.

Sampling

As in all studies, sampling is a critical design consideration. Plausible rival hypothesis are often suggested by examining the sampling plan of a piece of research. In cross-cultural studies, rival hypothesis are always suggested since the differences found may be attributed to dissimilar selection methods or disparate qualities of samples (ie. age, socioeconomic status, etc.). In much cross-cultural research there is no sampling plan; an experimenter is likely to use people who are talkative, cooperative or who are readily available. Even when random or stratified samples are attempted, the necessary demographic information may not be available and technical problems of not-at-homes or differential refusal rates may plague interpretations (Brislin et al., 1973).

Random sampling is rare in cross-cultural studies. When it is undertaken, the costs are great. Even when methodological

problems are overcome, the random samples themselves may be unequal in terms of socioeconomic or other cultural factors. Thus rival hypotheses are suggested even when random sampling is used. This does not mean that random sampling is worthless. It may be valuable in eliminating some kinds of systematic bias such as when only cooperative people are used. But whether sampling is random or non-random, the alternative hypotheses will have to be examined and eliminated.

In discussing cross-cultural random sampling, Lonner & Berry (1986) mention three general "rules." The first is that a representative sample may only be possible when the population is extremely homogeneous and there exists some accurate and up-to-date central registry that can be used as a sampling framework, something which is usually unavailable in developing nations. The second is that the best samples in cross-cultural research are those that result from the most careful attempts that the circumstances permit to approximate the kind of sample needed to permit the proper execution of the research. The use of non-random sampling in cross-cultural research is almost a given due to "indefinite populations, unavailable sampling frames, small budgets, lack of time, inexperienced personnel, pressure for results, and the like" (Warwick as cited in Lonner & Berry, 1986).

In stating their third sampling rule, Lonner & Berry first list a number of non-random sampling techniques found in the literature.

1. Samples of convenience. Individuals are taken from some convenient collectively such as a school or factory. Their accessibility makes them a very cost-effective sample. This technique departs to a degree from true representativeness. Because flux of time and situation will alter the configuration of such samples, they are not replicable.
2. Bunch or grab samples, also called fortuitous or haphazard samples. Individuals are selected in bunches because they are convenient and fit into the research scheme. Representativeness is a secondary consideration in this kind of sampling.
3. Judgmental sampling, also called deliberate, purposive or selective sampling. In this case, the researcher uses judgement in selecting individuals who are instrumental in gathering certain kinds of data or testing certain hypothesis.
4. Expert choice samples. This kind of sampling is similar to judgmental sampling but uses an "expert" instead of the researcher to select the sample.

While recognizing that random sampling is the ideal method, any of these alternative non-random sampling techniques may be the best approach possible under the circumstances. But when they are used, rule number three comes into effect which states that "When circumstances warrant departure from random sampling procedures, be as precise as possible in detailing the procedures you used, and make special note of any factor(s) which influenced the present sampling procedure but may not affect future procedures" (pp. 88).

Although sampling is always of critical interest when considering validity issues, it is apparent that cross-cultural research is faced with some special challenges which can not always produce the advantages associated with random samples. Assuming methodological problems can be overcome, cultural factors may render the samples less than equivalent. If the three rules described above are followed, however, the best sampling procedure under the circumstances should emerge and its rationale be described for analysis by others. While non-random sampling may limit the generalizability of the results, compensating for alternative hypotheses raised by the sampling plan will greatly strengthen the study.

Instrumentation

Social and behavioral scientists often use survey instruments to collect data. When conducting cross-cultural studies, these instruments are either (a) "standard" questionnaires developed in one culture (usually an industrialized nation) and used in another, or (b) an instrument developed by the researcher for a specific cross-cultural study. In either case, the instrument may need to be translated. Translated instruments run the risk of item non-equivalence in comparative studies or of item inappropriateness when standard scales, developed in another context, are used.

The use of standard questionnaires have the advantage that they are already developed (saving time and money) and that they have an established track record, thus increasing their respectability. Disadvantages include the fact that they have been developed in one cultural matrix and may miss important aspects of a phenomenon as seen by people in other cultures, and that their use also encourages intellectual timidity in researchers who might otherwise make a more original contribution. Thus the creation of new instruments, or the modification of existing ones to meet cultural requirements of the target group is suggested (Brislin, 1986; Segall, 1986).

Brislin (1973; 1980; 1986) has been instrumental in establishing guidelines for writing questions for use in cross-cultural situations. These guidelines are intended to assure that translators will:

1. Have a clear understanding of the original language item.
2. Have a high probability of finding a readily available target language equivalent so that they do not have to use convoluted or unfamiliar terms.
3. Be able to produce target language items readily understandable by the eventual set of respondents who are part of the data-gathering stage of the research project.

These guidelines are aimed at producing translatable English and are applicable to new item production or item modification.

1. Use short simple sentences of less than sixteen words.
2. Employ the active rather than passive voice.
3. Repeat nouns instead of using pronouns.
4. Avoid metaphors and colloquialisms.
5. Avoid the subjunctive, for example verb forms with "could," "would," or "should."
6. Add sentences to provide context for key ideas.
7. Avoid adverbs and propositions telling "where" or "when."
8. Avoid possessive forms where possible.

9. Use specific rather than general terms.

10. Avoid words indicating vagueness regarding some event or thing (eg. probably, maybe, perhaps).

11. Use wording familiar to translators.

12. Avoid sentences with two different verbs if the verbs suggest two different actions. (pp.144-149)

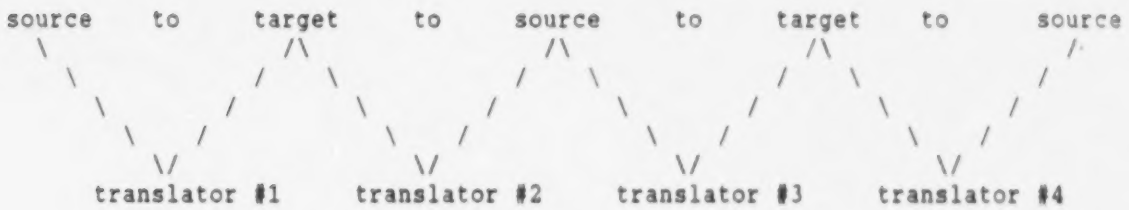
The use of translatable English in item modification or the creation of new items and questionnaires is an important aspect of cross-cultural instrumentation. While focusing on this technical aspect of instrument development, it is important to mention that standard procedures for assuring external validity and reliability need to be followed as well. Otherwise, the instrument may be translatable but relatively worthless as a research tool. While the focus here is on producing English language instruments as the original medium, this should not be interpreted as an attempt to exalt English as the research language. It is important to recognize, however, that English is the most internationalized language today and that most researchers around the world will be able to read and utilize the instrument if it is developed in this language. It is also more feasible to translate from English to almost any other language or dialect in the world than to work from another language.

When creating new items for inclusion in new or old instruments, Brislin (1986) suggests that thorough library research first be undertaken in studying the target culture. The writings of anthropologists, sociologists, political scientists, missionaries and government officials should be sought. Another excellent library source of cross-cultural information is the Human Relations Area Files. These files contain documents catalogued by culture, author, author's discipline and also by concepts covered.

Brislin also suggest working with bi-lingual persons in item development. These people are conversant in both cultures and can often conceptualize items in both languages. By seesawing between the two cultures, the bi-lingual may be able to write equivalent items in both languages. He/she may also have important insights into possible response bias in the target culture and help to formulate questions in such a way that this hazard is avoided.

The concept of moving back and forth between cultures is key to any translation effort. This technique known as back-translation or decentering, uses translators from both languages to blindly translate back and forth their counterpart's work. Through repeated translations and back translations, equivalent items "survive" and can be considered "etic" since words were

found in both languages to conceptualize them. The procedure can be depicted graphically as follows.



Back translation is a very useful technique in cross-cultural instrument development, but as with any other instrument, the final product should be pretested to assure its usefulness in the context in which it will be employed. The main advantage to back translation is that it allows researchers a degree of control over the instrument development since they can compare the original and back translations and make inferences about the quality of the translation. Even when researchers are conversant in the target language, Brislin suggest a similar process be used with monolinguals. The researcher can write the original in the target language, take it through a series of monolingual rewrites, and then compare the final version to the original.

While on the subject of instrument development, Segall (1986), in a discussion on the design of cross-cultural attitude scales, mentions the importance of balancing the items between

those worded positively and those worded negatively. The item "Mexicans are warm and honest people," might be balances with one like "Mexicans are likely to try to cheat outsiders." On a Likert scale, answers to these two questions should be somewhat opposed, that is, if both are answered as "strongly agree," this might reveal a bias towards agreeing with all items.

Segall (1986) also mentions an interesting technique for scale analysis which he refers to as the "Guttman analysis."

> Create an a priori set of items, worded in both directions, administer them to a sample of respondents, arrange respondents in order and items in order, eliminate those items which persistently contribute disorder to the array, and, finally, live with reduced set of items as the best possible scale. Respondents are then re-scored on that best possible scale and the new scores constitute the best possible estimate of the respondents' attitudes. (p. 284)

This particular technique suggests itself in the absence of adequate prior reliability testing or as a double check on the accuracy of that process.

Once the instrument has been designed, translated and properly tested, its administration poses other problems the researcher should be aware of. The interview or questionnaire

procedure may be subject to respondent biases which have been creatively named by Pareek & Rao (1980).

1. The rudeness bias. Researchers forget that they are requesting valuable time of people in other cultures and plunge ahead without adequate knowledge of local norms and protocol.

2. The sucker bias. Respondents give nonsensical answers in the spirit of fun to see how seriously the researcher will take their silliness.

3. The I-can-answer-any-question bias. The deep-seated conviction by some respondents that all questions should be answered whether or not they have the knowledge or information asked for.

4. The courtesy bias. The respondents try to discover what the researcher is looking for and accommodate their answers accordingly.

5. The hidden premise bias. The respondents try to figure out what the researcher is *really* after. Government sponsored research is seen as an attempt to extend governmental control.

There is no single solution to dealing with these potential sources of bias. As has been suggested, the researcher should be

as knowledgeable as possible about the target culture and anticipate the kind of bias that may occur. Persons from the target group working on instrument development may also be able to anticipate some of these problems and guide the researcher in developing and adequate strategy. If the instrument is tested in an "real" situation, biases may be identified at that time and compensated for in the final administration.

Summary

Conducting research in a cross-cultural setting is attractive to scientists from many disciplines for a number of reasons. But as has been expressed, it is not simply a matter of "business-as-usual" in another setting. The researcher needs to deal with the "normal" sources of bias and threats to validity common to all research design and administration, but in cross-cultural research, these are greatly exacerbated by the foreign field setting. In addition, there are a number of methodological problem areas which are never encountered in intra-cultural studies.

By providing an overview of possible sources of bias and threats to validity while conducting cross-cultural research, it is hoped that the cross-cultural researcher, particularly the novice, can avoid some of the pitfalls inherent in this activity.

While beyond the scope of this paper to present in-depth development of techniques and methods to overcome these difficulties, the brief outlines presented should provide an orientation to procedures used by experienced researchers and guide the reader to their sources. The challenges to overcome are great but careful attention to the issues addressed in this paper should produce a worthwhile and rewarding research effort.

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